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LECTURES.

Abstract of a Lecture on the Relation of Bacteria to the Chemical and Physical Processes which constitute the Life of the Higher Animals. By J. BURDON SANDERSON, M.D., Prof. Phys. in Univ. Coll.

As introductory to my subject, I must give a short account of the natural history of the organic forms which we now, following high botanical authority, group together under the term bacteria. Secondly, I shall consider the question of their general influence on the processes of life of the higher animals, and the mode in which this is exercised. This will prepare us for discussing the question how far they act as morbific poisons, or as the vehicles of such poisons, or are characteristic of specific diseases. I would remind you that the question we have before us is not that of the origin of bacteria, nor that of their place in nature, but the more restricted one of their influence on ourselves, and on the lower animals whose life, whose health, and whose diseases are but the counterparts of our own.

It is because they are constantly invading our bodies and attacking the living protoplasm by which the work of life is carried on in our tissues, that we, as pathologists, take an interest in their natural history. It is for this reason that questions relating to them, which at first sight seem recondite and remote, have a direct and immediate bearing on practical

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questions. For if, as I shall endeavour to show, they are constantly present to us—constantly mixing up their life with ours—we cannot wisely remain ignorant of their nature. We do not aspire or profess to be botanists; but, if plant-forms and plant-life associate themselves with the very processes which it is our business to study, we must become botanists for the occasion.

In the short introductory account I shall give of the forms of bacteria, I shall follow Professor Cohn (*Ueber Bacterin, die kleinsten lebenden Wesen*, Berlin, 1872), as being the only botanist of high authority who has given special attention to the subject. For the remainder of what I have to say, I am myself answerable. I shall, however, make it my endeavour not to vindicate the views which I have advanced myself, or which others may have attributed to me; but to place before you a general view of the work that has been done during the four years which have elapsed since the subject began to occupy the serious attention of pathologists.

Morphology and Natural History of Bacteria.—The first fact that I shall advance with respect to bacteria is, that they are the smallest and least organized of all living beings. As regards size, it is best to judge by comparison with objects with which we are microscopically familiar. The most common rod-like forms are in length about one-third of the width of a blood-corpuscle; *i. e.*, about $\frac{1}{3000}$th of an inch—so small that, if we examine a liquid containing them, with the ordinary magnifying powers used for histological observations, we can scarcely be said to see them to any practical purpose. It is necessary to have recourse to the best microscopes and the highest powers, if it be desired to observe them in such a way as to arrive at useful results.

What grounds have we for stating that they are the lowest organisms? One is, that they present only very slight differentiation of parts; but in this sense they are certainly not simpler than many other forms that might be referred to. The chief ground for the statement lies in this, that they are much less *specific* in their characters—much more under the influence of the conditions under which they originate and are developed—than organisms of any other class. Just as in the higher animals and in man himself we call those functions lowest which are most completely automatic—*i. e.*, most completely under the guidance of known conditions—so also, as regards form, we recognize that while all animal and vegetable forms, even the highest, are moulded by circumstances to fit their places in the economy of nature, this moulding power—this adaptation of form to circumstance—becomes more and more obvious the lower we descend in the scale of development.

The next fact relates to the *habitat* of bacteria—to the medium in which they live—water. They inhabit water either as such in the ordinary sense, or in the various conditions recognized as *moisture*, whether occurring on damp surfaces or as filling the interstices of solid bodies, which bodies, when so impregnated with water, are said to be damp. Those who are familiar with chemical work know that this quality of dampness goes a great deal further than the popular notion of it; that many things ordinarily called dry yield, when subjected to the drying processes commonly used in the laboratory, evidences of being really moist. Consequently, moisture, regarded as a limiting condition of bacterial life, is a very wide and comprehensive one.

From this statement, it must not be understood that bacteria do not exist in the atmosphere. But their existence there in an active form strictly depends on moisture. They attach themselves, without doubt, to those minute particles which, scarcely visible in ordinary light, appear as motes in the sunbeam, or in the beam of the electric lamp. It is by the agency of these particles that they are conveyed from place to place.

Notwithstanding that the word bacterium means a rod, and that many of the forms to be immediately referred to are not rod-like, I am obliged to use it, because it is used by others as a general term for the whole group of organisms known to botanists as *Schizomycetes*.

Bacteria have, as a rule, two states of

existence—a state of activity and a state of rest. When a liquid teeming with bacteria in the active state is observed under the microscope, the attention is so riveted, that it is an effort to take away the eye from the instrument. The movements have been often described. In the case of rod-shaped bacteria, the axial movement in which the rod advances or retreats in the line of its axis, the direction being frequently reversed, is the most common. This kind of locomotion occurs often by fits and starts, the body remaining in the intervals quite still, or assuming a pirouetting or spinning movement. In all rod-like bacteria, it is probable that the progressive or axial movement is associated with rotation, for, in observing the motion of vibrios, it is easy to see that they, in progressing, twist round the axis of the spiral. When this is the case, it looks as if the filament were executing a wriggling motion, *i. e.*, as if its body were contractile; but this is obviously deceptive. The mechanism of the motion is as little understood as those of *Oscillatoria*, which it closely resembles. It must be carefully distinguished from the passive motions which are exhibited by all particles of size comparable to that of bacteria, when suspended in a liquid of which the density does not differ very widely from their own. We shall find afterwards that certain forms of bacteria appear to be motionless in all stages of their existence.

Rod-bacteria multiply by repeated bisection. This mode of multiplication goes on continuously, the rate varying with the conditions of life, and particularly with temperature. Under favourable circumstances, it is enormous. From actual measurement of the time which elapses in a single case between a bisection and the division of each half into quarters (in other words, of the longest time that a single bacterium remains without dividing), it has been ascertained that the interval in question, in common bacteria, is about an hour. This being the case, it is easy to compute that every single bacterium must produce 16,777,220 individuals in twenty-four hours. Putting it otherwise, the progeny of a mass of bacteria weighing $\frac{1}{2400}$ grain would, at the end of a day, weigh a pound.

In the largest forms of bacteria, it can be made out that each individual consists, not, like an animal cell, of one sort of living substance,[1] but, like a plant-cell, of a protoplasmic interior inclosed in an envelope of cellulose, by virtue of which the bodies of bacteria are enabled to resist the action of reagents, particularly weak acids and alkalies, which dissolve protoplasm. In those bacteria which separate from each other, as soon as they assume the form of distinct individuals, the more stable external part presents itself as a mere envelope; but when, as very usually happens, the progeny which results from a continuous succession of divisions remains in a state of aggregation, the envelope appears to become identified with the "gelatinous" matrix by which they are held together. This observation was made more than twenty years ago by Cohn, who devised the term zooglœa as a general designation for the clumps, or, as Hallier calls them from the mode of aggregation above described, "colonies." You will find that this term is now very constantly used by writers on the subject, for these gelatinous masses occur under a great variety of circumstances. In form, they are mostly either spheroidal or membranous, and have this one characteristic which is noteworthy, that their structure is such as to indicate that they grow at their surfaces or margins, not interstitially. Thus, for example, in the spheroidal forms, which, as seen under the microscope, present a more or less circular outline, it is obvious that the multiplication of individuals is most active near the edge. The membranous forms, on the other hand, often present a growing outline in one direction, a disintegrating one in the other.

We are not able to state that the process of repeated division into two is the only one by which bacteria come into ex-

[1] In using the expression "one sort of living substance," I must guard against being supposed to mean that any kind of living protoplasm is homogeneous. Hyaline though it may appear, we are not able at present to assign any limit to its complexity of structure.

istence. When we watch a liquid in which they are beginning to appear as if spontaneously, where none were visible a few minutes before, all that we can make out is, that a nebula presents itself; and that here and there, in the previously homogeneous liquid, particles exist which, although their form is at first indistinguishable, eventually become recognizable as bacteria. As to the way in which this happens, it is difficult to speak positively, for with reference to it no evidence is admissible excepting such as is founded on direct observation—*i. e., on watching the process in a single instance for many hours*—fixing the eye on a single bacterium, a thing weighing two-billionths of a grain, and measuring one-eighth-thousandth of an inch, and not quitting it till it divides and divides again. Klebs has attempted to do this, but I will not detain you with an account of his researches.

From what I have said as to their structure and ordinary mode of multiplication by division, it is obvious that bacteria resemble plants, and particularly the Oscillatoriæ, more than animals. The proof that they are plants rests on what is known as the nature of the chemical processes which constitute their life, to the consideration of which we must now pass.

How do bacteria act on the medium in which they live? On this subject, we owe the foundations of our knowledge to Pasteur. The first fact is, that bacteria act on the media in which they live, not as animals, but as plants. Like plants, they derive the nitrogen that they use to build up new albuminous compounds, not from previously existing albuminous compounds, but from ammonia. Like other colourless plants, they derive their carbon apparently from any carbon-compound except carbonic acid, by dissociation of its elements. Like plants, they require certain inorganic constituents to be present in their soil, particularly potash and phosphoric acid.

As regards the assimilation (or fixation) of carbon, Cohn's researches have very materially advanced our knowledge since the publication of Pasteur's work. It was obvious that bacteria did not decompose carbonic acid under the influence of sunlight, by the same wonderful process by which that dissociation is performed by green plants. This they could not do in the absence of chlorophyl. But in this respect they were not in any different position from the fungi, or even from the colourless phanerogamic parasites—as, for example, the bird's-nest orchis.

Pasteur's investigations related, not to bacteria or to the organic forms which are associated with the process of putrefaction, but to the yeast-plant. With reference to that organism, he proved that it derives its nitrogen directly from ammonia. He proved at the same time that the cellulose of which the external part of the yeast-cell consists, is derived from grape-sugar, and cannot be formed in the absence of that body. He did not extend the application of these facts to the organisms of putrefaction; but it came to be assumed that what was true of the yeast-plant would also be true of the others—viz., that, although ammonia was a sufficient source of nitrogen, some hydrocarbon must be present to yield carbon.

Cohn showed this assumption to be a mistaken one, by applying to the organisms of putrefying liquids the same mode of investigation that Pasteur had before applied to the yeast-plant (Cohn, *Beiträge zur Biologie der Pflanzen*, Heft ii. pp. 191–202). That method (since known as the method of cultivation) consists in first finding a liquid of known chemical composition in which the plant to be investigated grows vigorously, and then gradually modifying the composition of this liquid by the elimination of one after another of its ingredients, until at last a mixture is obtained in which the greatest possible simplicity is combined with the greatest possible adaptedness as a soil to the requirements of the particular organism; adaptedness being judged of by rapidity of growth. Such a liquid, as regards the yeast-plant, is that known as Pasteur's cultivating fluid. Its composition stands for the fact that sugar and certain crystalline salts are all that is wanted for the nutrition of the yeast-plant.

In the earlier experiments as to the

cultivation of bacteria, *e. g.*, in those which I made in 1871, it was assumed that Pasteur's liquid would also be the most suitable liquid for bacteria. During the same year, however, Cohn, observing that, although bacteria flourished in it abundantly, it was very difficult to prevent the growth at the same time of the yeast-fungus and of penicillium, *i. e.*, common mould, left out the sugar, and found that the bacteria flourished better without it than with it. His next step was to substitute for the solution yeast-ash (a material which is troublesome to prepare) a solution containing the required salts in about the proportion in which they are ascertained to exist in the ash of the yeast-plant. The liquid which we now use as a "cultivation liquid" for bacteria is prepared by dissolving half a percentage each of potassic phosphate and magnesic sulphate in water having a trace of calcic phosphate in suspension,[1] and then adding as required a further percentage of ammonic tartrate, and of course boiling the mixture. In this liquid, bacteria grow rapidly—a fact which proves not only that they are able to take their nitrogen from ammonia, but that they can also derive carbon from the tartrate—the only carbon-containing body which is present. Although, however, it supports the life of bacteria so completely, that they not only subsist in it, but multiply with enormous rapidity under favourable conditions of temperature, it never originates them, if it have been boiled. It can, indeed, be very easily kept for indefinite periods without change, remaining absolutely transparent and barren, provided that care be taken to protect it from contamination. It is, however, much better practically to keep the solution of inorganic salts, adding the tartrate in the proper proportion for each set of experiments.

In order to determine the development of bacteria in a liquid of this kind, all that is necessary is to touch it with a "damp" surface—a glass rod, a thread of cotton or silk—*any object which, having been exposed to the atmosphere, has not since its exposure been dried by heating it to a sufficient temperature.* The result may be equally well attained by allowing a particle of dust or a drop of water, or common distilled water, to fall into the liquid. All these liquids, although they contain no organic forms which can be recognized even with the aid of the highest powers, yet contain that which, when added to solution of ammonic tartrate, determines its decomposition, and the building up of its nitrogen and carbon into higher combinations—in a word, vegetation. I call this process quasi-spontaneous. It is not spontaneous, for this simple reason—that it is dependent on conditions which are so far known, that it is possible to control them with perfect certainty.

I may add, that it was by experiments such as those to which I have just been referring that I was enabled to show, in 1871, that moist surfaces and particles of solid material in suspension in the air play the chief part in the propagation of bacteria; *i. e.*, in the conveyance of the material out of which they spring from one soil to another. This inference has now been confirmed—I think I may venture to say, established—by very numerous observations.

Influence of Bacterial Vegetation on the Process of Putrefaction.—In referring to Cohn's classification of the organisms which constitute the group of Schizomycetes according to their form, I stated that this classification was chiefly of value for the purposes of description, and that the exact similarity of any two forms cannot here be taken as evidence of their organic continuity any more than their dissimilarity affords indication of the absence of intimate relation between them; for the influence of environment over organisms such as bacteria is so great, that it seems as if it were paramount; the surrounding conditions claiming a power not merely of moulding the organisms into conformity with themselves, but even of originating it.

For reasons on which it is unnecessary to enter here, we do not admit the latter

[1] The calcic phosphate is best prepared by precipitating solution of calcic chloride with common sodic phosphate, taking care that the former shall be in excess. This precipitate splits on boiling into a soluble and an insoluble phosphate.

part of this claim; but it is a principle of fundamental importance in dealing with the question we have now before us —that of the agency of bacteria in septic processes—that the influence of circumstance over form is practically so powerful, that, in considering the relation of bacteria to septic processes, we shall do best if we regard the appearance of particular forms as mere links in the chain of events of which the process consists.

To illustrate this, it will not be a waste of time to refer for a moment to the singular fact discovered a short time ago by Professor Cohn, that, under conditions as yet quite unknown, colouring matters come into existence in albuminous liquids when left to themselves at ordinary temperatures; and that this development of pigment is associated with the development of bacteria. Not long ago, having left a "solution" of ordinary egg-albumen in a cupboard in the laboratory, I found that the liquid, which had no putrid odour, possessed an intense indigo-blue colour. Being acquainted with Cohn's observations, I at once guessed what had happened, and found, on microscopical examination, that the blue scum on the surface of the liquid consisted, first, of colourless bacteria, resembling in form those met with in septic liquids; and, secondly, of blue pigment-particles of extreme minuteness, which appeared to be imbedded in the interstitial gelatinous material by which the organisms were surrounded. The process continued for some time; but eventually the liquid was inadvertently allowed to dry. On inoculating a similar albuminous liquid with the dried residue, I was unsuccessful in reproducing the process; but Cohn, who came upon several of his colour-producing bacteria in a similarly accidental way, was able to perpetuate the process by repeated transmissions.

I mention this observation merely to illustrate what I have been saying as to the relation between the two collateral processes—between the vegetation-process, on the one hand, and that of the formation of a characteristic chemical product, on the other. Here we have no sufficient reason for saying either that the production of colour is the cause of the development of bacteria, or that the bacteria are the agents in the production of the pigment. All that we observe is, that the two phenomena begin, continue, and end together; we regard them, therefore, as collateral characteristics of one and the same process.

As the result of innumerable observations, we know that, as a rule, those changes in albuminous liquids, which we familiarly recognize as constituting putrefaction, are accompanied by the presence in such liquids of characteristic vegetable organisms. We also know that the two processes—viz., the chemical changes and the rapid vegetation—go on *pari passu*, and that the same circumstances which favour the growth and multiplication of organic forms also favour putrefaction.

As to the meaning of these facts, there is some difference of opinion even among competent naturalists. All are agreed that the relation between the chemical process and the vegetative process is an intimate one: but somewhat discrepant views prevail as to its nature.

The fundamental experiment by which the intimacy of the association is proved is this. When a tube or flask is partly filled with water containing albuminous matter in solution or suspension, and subsequently boiled for a sufficient length of time, and closed hermetically during ebullition, no putrefaction occurs under any circumstances; but if, after such a tube has been heated and allowed to cool, a drop of distilled water, or any other exposed fluid, be added before the tube is again sealed, putrefaction follows, provided that the preparation is kept at the ordinary temperature.

As I have already hinted, there are some naturalists who would not agree to the terms in which I have stated this result. They would rather substitute for the words "under any circumstances" some such expression as "in 9999 cases out of 10,000." For our present purpose, it makes little difference whether the statement be accepted absolutely or not. But there is another point relating to the origin of bacteria in albuminous liquids of

much greater importance, which the discussion about abiogenesis is apt to conceal or throw unduly into the background. It is a question which arises directly out of the results of the "cultivation" experiments to which I have just referred. These experiments show that bacteria are able to derive the whole of the carbon they require for the building up of their protoplasm from compounds of great chemical simplicity, which have never formed part of any living organism, and seek for no higher source of nitrogen than ammonia. This being the case, it is more than probable that, when the growth of bacteria goes on in association with septic processes, they derive their nitrogen and carbon, not from the albuminous compounds themselves, but from the ultimate products of their disintegration. This being the case, we must regard bacterial life (in so far as it consists in the building up of new protoplasm) as a process consequent on the chemical process of putrefaction with which it is associated. For disintegration must already have proceeded as far as to the production of ammonia —in a word, must have proceeded to the last stage—before the new integration could commence.

While admitting this, it is important clearly to see that the admission does not in any way render it more or less probable that bacteria are the efficient causes of putrefaction—that without which putrefaction could not take place—for there is nothing which forbids us to regard bacterial vegetation as connected (if I may so express myself) with both ends of the chemical process of disintegration of putrescible material at the same time; not the shadow of an objection to the assumption that, on the one hand, bacteria derive material for the integration of their protoplasm from the products of *dis*integration of the soil in which they flourish, and, on the other, that they produce the ferment by which disintegration is determined. There is nothing, in short, against their standing to the chemical process at the same time in the relation of antecedent and consequent.

If it should seem to you that this is a too vague way of dealing with the subject, I would ask you to consider that the question is one in respect of which caution is more than usually necessary; for, while theoretical explanations offer themselves freely on every side, experimental investigation is beset with peculiar difficulties. As regards the main question, that of the agency of bacteria in producing putrefaction, there are two extreme views, which are both clearly mistaken—the one which asserts that they have nothing to do with the septic process; the other, that the chemical phenomena of putrefaction are the mere accidents of a peculiar kind of vegetation. The truth lies without doubt between these two opposed theories; but it is not to be got at by an ingenious reconciliation of the one with the other, but by honestly opening the mind to the facts as they stand, in the confidence that, if they are allowed fair play, they will eventually shape themselves into a general conception, in accordance with their true relation. So long as uncertainty exists, there is nothing to be so much avoided as that sort of clearness which consists in concealing difficulties and overlooking ambiguities.

[Since this lecture was delivered, an important contribution to the elucidation of this question has been made by Dr. Hillier of Berlin ("*Der Antheil der Bacterin am Fäulnissprocess*," *Centralblatt*, No. 54, Nov 14, 1874). Starting from the fact observed by himself (to the accuracy of which I can testify from my own observation), that, in the "alkaline fermentation" of urine, the splitting of urea into ammonic carbonate is not in relation with the abundant development of septic bacteria in the liquid, he made experiments which showed that in urine, bacteria do not derive their nitrogen from urea, but from other sources; and that, if ammonic tartrate be added to urine, the multiplication of bacteria goes on with great activity without any diminution of its acid reaction, *i. e.*, without any decomposition of its urea. From this result, he proceeds to similar experiments as to the influence of bacteria on egg-albumen. Having first ascertained, by careful observations, the fact familiar to the housewife that fresh eggs "turn" when

put into the same basket with rotten ones, and can therefore be infected through the air, he injects a "cultivation liquid," containing bacteria, but not putrid, into the albumen of a fresh egg, with the aid of a subcutaneous syringe. The egg remains unaltered. He therefore thinks it impossible to identify multiplying bacteria with the "septic ferment;" with reference to which he thinks that the experimental investigations of the last few years have proved nothing more than "it is a particular substance which either exists suspended in the air, or adheres to objects, and that it can be separated from the air by filtration, and can be destroyed by the temperature of ebullition." With this final statement of the case I perfectly agree, and am not at all disposed to quarrel with him because he goes on to suggest that the "particles" of which the septic ferment consists are not living beings, but *Protëinsplitterchen.*]—*Brit. Med. Journ.*, Jan. 16, 1875.

CLINICS.

Clinical Remarks on the Simulation of Dislocation into the Axilla after Union of Fractures through the Tuberosities of the Humerus. By J. HUTCHINSON, Esq., Surgeon to London Hospital.—Mr. Hutchinson devoted this lecture chiefly to an explanation of the resemblances and differences between unreduced sub-coracoid dislocation of the humerus and united fractures of the bone through the parts above the surgical neck. The remarks were elicited by the case of a middle-aged man who was brought into the theatre. Several months ago this man had received a severe injury to his right shoulder. The surgeon who attended him in the first instance thought that the parts were only severely bruised. Three weeks after the accident he was told, at a provincial hospital, that the bone of the arm was fractured near the shoulder; and again, some time later, after he had left the hospital, the diagnosis of unreduced dislocation of the humerus was made. Subsequently an attempt at reduction under chloroform was made. He was next sent by his surgeon to the London Hospital, to know whether anything further could be done to put it in its place again. Some of the signs of sub-coracoid dislocation were absent, while those that were present existed in only a modified form; thus, while there was no inability to bring the elbow to the side, and no stretching of the deltoid muscle, there was considerable flattening of the shoulder, with marked hollowness under the acromion and a prominence of bone in the axilla. The last two symptoms were, however, less marked than in cases of dislocation; and while the altered outline of the shoulder was in great part due to atrophy of the deltoid, the prominence in the axilla was caused in part by general thickening of the upper part of the humerus, but chiefly by a partial displacement of the head subsequent to the fracture.

Mr. Hutchinson conjectured that the bone was broken irregularly through the tuberosities, perhaps comminuted; that the shaft was pushed upwards slightly, and had tilted the head of the bone in such a way that its articular surface thenceforth looked inwards. Such a displacement would produce a slight rounded prominence in the axilla, but would not prevent the arm from being brought to the side; it would also produce, after union, considerable general thickening of the upper part of the humerus. The atrophy of deltoid, and consequent simulation of the acromial prominence present in dislocation, was to be accounted for by direct injury to the circumflex nerve.

The wasting of the deltoid, and therefore the flattening of the shoulder, do not come on until many weeks after an injury of this kind; consequently, it is generally when the patient goes to a fresh medical man two or three months after the accident that the diagnosis of unreduced dislocation is made. Mr. Hutchinson believes that this mistake (aided as its commission is by the bony prominence in the axilla) is not unfrequently made, and that thus the reputation of the surgeon who first saw and treated the case is often seriously damaged. He related a case in which he had, he believed, made the mistake in question. An attempt at reduction

of the supposed dislocation was followed by very decided crepitus about the shoulder, but, although the bone was thus broken afresh, the patient in the end gained much more movement at the shoulder than he had enjoyed before the traction had been applied; probably some old adhesions were broken down.

In another case the state of things was ascertained by dissection. A lunatic had died in an asylum about a year after having been treated for what was diagnosed as fracture in the upper part of the humerus. On the post-mortem table a conspicuous hollow was noticed under the acromion, and so deceptive were the conditions that the resident medical officer who had treated the case manifested much anxiety lest the dissection should reveal a piece of bad surgery and prove that he had overlooked a dislocation. This was, however, not the case. The dissection showed a united fracture through the tuberosities, with deformity so as to make the head of the humerus somewhat resemble that of the femur, and some displacement downwards in the glenoid, but no true dislocation. The deltoid was wasted, but, unfortunately, the opportunity for dissecting the circumflex nerve was neglected.

The examination of this case had helped Mr. Hutchinson to a confident opinion as to the true nature of the injury on many subsequent occasions, and he had, he believed, not infrequently prevented attempts at reduction which would have been useless, as well as saved the surgeon first consulted undeserved loss of repute. In reference to the latter point he insisted strongly on the importance of the knowledge *that old united fractures, though the shoulders of the humerus very often present remarkable deformity, closely simulate dislocations.* In the production of this simulation, paralytic atrophy of the deltoid plays, he said, a very important part. This paralysis may be partial or complete, and its frequency after these fractures can excite but little surprise if we remember the close proximity of the circumflex nerve to the bone, and its consequent risk of injury.

A case was next shown, side by side with the above, in order to illustrate the effect of total atrophy of all the muscles around the shoulder joint. In these cases the acromion becomes abnormally prominent by reason of the muscular atrophy, but there is no prominence of bone in the armpit, no difficulty in bringing the elbow to the side, nor any stretching of the fibres of the deltoid. The capsular ligament elongates and allows the head of the bone to leave the glenoid cavity, but the normal relation between the two articular surfaces can at once be restored by lifting the humerus upwards. The paralysis, which was total and affected the whole extremity, in this case followed a severe fall on the shoulder, in which the clavicle was broken (without much displacement), and the brachial plexus probably torn across.—*Med. Times and Gaz.*, Jan. 30, 1875.

MEDICAL NEWS.

DOMESTIC INTELLIGENCE.

Posterior Staphyloma, etc.—We are informed that Dr. WILLIAM THOMSON, of Jefferson Medical College, has observed a large number of cases of myopia, in which there were posterior staphyloma, choroidal atrophy, posterior polar cataract, separation of retina, etc., which conditions appear to be induced by astigmatism. He has often noticed cases of high degrees of simple myopia which by suitable glasses obtained the full acuity of vision. The eye-ground in these cases was generally normal in appearance, but the eyeball was elongated in its antero-posterior diameter, most probably by inheritance. Most of the myopes cannot, however, be fully corrected by spherical glasses, and present white crescents at some part of the margin of the optic nerve, with a dragged appearance of the vessels. By the erect method of examination with the mirror, Dr. Thomson finds that astigmatism may be detected, and its meridian ascertained, by a close observation of the line of drag, and the position of the crescent; and after correction by cylindrical glasses, he finds that a constant relation then exists between the direction of the crescent, and the axis of the glass.

Our readers may expect to find in the forthcoming number of the *American Journal of the Medical Sciences* a full account of these interesting observations, and of the anatomical, physiological, and clinical facts upon which they are based.

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Impaction of Coin in Œsophagus.—Dr. Chas. B. Brigham relates (*Western Lancet*, March, 1875) a remarkable case in which a silver quarter of a dollar was impacted in the œsophagus of an infant æt. nearly three years, and which he extracted with a sponge probang six months and two days afterwards.

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American Medical Association.—The twenty-sixth annual meeting of this Association will convene in the city of Louisville, Kentucky, on Tuesday May 4th, 1875, at 11 o'clock A. M.

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Medical Graduates in 1875 (continued from p. 56).

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University of Nashville and Vanderbilt University.—The commencements of the Medical Departments of both these Universities, we see it stated, were held at the same time and in the same place, and what is somewhat curious, a number of candidates received diplomas from both institutions, the University of Nashville conferring the degree on 60 and Vanderbilt University on 61 candidates, making a total of 121. But from the circumstance above stated the actual number of graduates was 74, and not 58, as stated in our previous number.

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Hospital of the University of Pennsylvania.—Dr. James Tyson has been elected Professor of Pathology; Dr. Lewis A. Duhring Professor of Dermatology, and Dr. H. C. Wood Professor of Diseases of the Nervous System, in this Institution.

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Medical Department of Harvard University.—We are gratified at seeing it announced that this school is about to take another step for the advancement of medical education. No student will, after the beginning of the school year of 1877, be admitted to the classes without passing an examination, except such as can present a degree in letters or sciences from a recognized college or scientific school.

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Appointment.—Dr. D. W. Cheever has been appointed Professor of Clinical Surgery in the Faculty of Medicine, Harvard University.

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Bellevue Hospital Medical College.—Dr. Frank H. Hamilton has resigned the chair of "Practice of Surgery, with Operations and Clinical Surgery," in this Institution.

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Association of Ex-Confederate Surgeons.—The second meeting of this Association is to be held in Richmond, Va., on the 19th of October next, at 11 o'clock A. M.

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Obituary Record.—Died, on the 31st March, 1875, at his residence, Morton, Delaware Co., Penna., in the 80th year of his age, D. Francis Condie, M.D., formerly of Philadelphia.

Dr. Condie was well known by his numerous and valuable contributions to medical literature, and was distinguished for his extensive professional acquirements. His work on the Diseases of Children, the best at the time, was much esteemed, and went through four editions. Besides editing a number of medical works, as those of Churchill and Watson,

etc., to which he made important additions, he was a large contributor to the medical journals, particularly to the *North American Medical and Surgical Journal* and to the *American Journal of the Medical Sciences*. He was an early advocate of the temperance movement, and in conjunction with the late Dr. John Bell, edited a journal (*The Journal of Health*) devoted mainly to the promotion of that cause. For upwards of fifty years he was engaged in extensive practice, which he was obliged to relinquish about four years since in consequence of being crippled by a fall on his hip, when he removed to a farm a few miles from the city. Until a recent period, however, he was busy with his pen, and contributed a number of valuable articles to the *American Journal of the Medical Sciences*.

FOREIGN INTELLIGENCE.

The Germ Theory of Disease.—There has recently been an interesting discussion at the Pathological Society of London, on the relation of Bacteria and Allied Organisms to the Virulent Inflammations and to the Specific Contagious Fevers. Dr. Bastian commenced by referring to the analogy which existed betwixt zymosis and fermentation, in that particles in each case set up processes which resulted in the reproduction of other particles identical with the first. He contrasted Pasteur's views with the transferred eremacausis of Liebig, inclining to the views of the latter. The contagium multiplied within the body in a manner which might be compared to organic growth and multiplication. Bacteria were not found in the blood of healthy individuals; yet, in from eight to forty-eight hours after death, they were found in abundance. He contended that bacteria had a casual connection with infection, and that they were not mere "carriers of infection," but were for the most part actual pathological products engendered within the body; that, in fact, there was a bacterial degeneration which claimed to rank side by side with fatty degeneration. After expounding his views with much skill and detail, he was followed by Dr. Burdon Sanderson. Dr. Sanderson declined to go into the germ theory in its entirety, but confined himself to the negative facts that bacteria identical with those found in morbid tissues, might be introduced into the body without any ill effects; and to the positive facts that, in destructive inflammations, bacteria were found only in the outer zone of the disease, where it was spreading, and not in the central parts; and that in relapsing fever spirillæ were only found during the febrile paroxysm and not afterwards; and that in erysipelas and sheep-pock, the bacteria were found not in the vesicles, but in the lymphatics of the skin. These facts, he thought, were opposed to Dr. Bastian's views.—*Brit. Med. Journ.*, April 10, 1875.

Action of the Septinous Poisons on the Production of Fever.—Dr. B. W. Richardson, in a recent lecture before the Society of Medical Officers of Health, offered a theory of the action of the poisons of contagious diseases in the production of symptoms which mark those diseases, and especially the symptoms of fever. In a lecture delivered before the same Society, in the year 1865, the author detailed his original experiments on the poisonous action of an organic substance he had obtained from sero-sanguineous fluid that had been removed by Mr. Spencer Wells from the peritoneal cavity of a woman suffering from pyæmia after the operation of ovariotomy. To this organic poisonous substance he (Dr. Richardson) at that time gave the name of septine, and he showed not only that it would communicate fatal disease to an inferior animal, but that the same disease could be communicated from the secretions of the primarily infected animal to other healthy animals in direct series. He also showed in the same research that the organic poison could be made to combine with acids so as to form salts which retained the poisonous qualities of the original substance. That a similar poisonous substance is present in decomposing blood, and that the poison can be transmitted in a series of animals, have since been confirmed by the independent researches of many physiologists, and the fact may be considered as fully

established, although great difference of opinion still remains as to the nature of the organic product, some assigning to it purely physical, others vital properties.

Dr. R. has ever since his researches in 1865 been engaged in the inquiry how these organic poisons produce their peculiar effects, and the result is to the effect that the poisons exert their deleterious action through the property they possess of setting at liberty the oxygen of the blood in undue quantities in the extreme circulation. By way of experimental demonstration, Dr. Richardson took specimens of solutions of peroxide of hydrogen, and inoculated them with points of different organic poisonous animal substances—viz., decomposing blood, pyæmic poison, pus, fibrine, and vaccine lymph. In the presence of all these bodies, oxygen began to be immediately liberated from the solution. Other organic substances—mucus, for instance—would not liberate oxygen, a fact which could be applied (as was shown by an experiment) for diagnosing mucus from purulent matter. Proceeding with his demonstration, Dr. Richardson charged blood with oxygen from the peroxide solution; then, by inoculating this blood and setting oxygen at liberty, he showed the temperature of the blood rose 4° Fahr. In his further observations, he explained that these organic disturbing substances are themselves decomposed in the process of the change they excite, and that they all seem to be derived originally either from fibrine or cellular tissue.

The conclusion arrived at, and which was advanced admittedly as a theory in its present state, is that the fever-producing poisons act in the way described by setting oxygen free in the blood in undue proportion. The theory, it was urged, explains the phenomenon of remittency. The accession of fever is due to the presence of the poison, but as the poison becomes destroyed the disturbance ceases until a new charge is locally produced and carried into the blood. The theory also explains the phenomena of those malignant contagious diseases in which death occurs very rapidly, with symptoms of asphyxia and convulsion. Here, it is assumed, the liberation of oxygen is sudden and complete, and the blood so surcharged with poisonous matter that it cannot duly absorb and fix oxygen in the pulmonary circuit. Thus an asphyxia commences from the blood itself.

Some therapeutical considerations brought the lecture to a close. It was shown that certain medicinal substances, among which quinine was specially named, counteract the different poisons in their power of liberating oxygen.

Our readers will gather from this summary that a new field of inquiry on the subject of the poisons of the contagious diseases is opened by these researches. That the inferences drawn support the physical side of the question may naturally be expected, coming as they do from an author who for so many years has been one of the most prominent exponents of the physical theory of contagion.—*Lancet*, April 3, 1875.

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Action of Salicylic Acid in Bacteria.—Dr. Ludwig Letzeritz has lately tested the effect of salicylic acid on bacteria and micrococci, by placing under the microscope portions of fluid containing these organisms in abundance, and allowing solutions of salicylic acid to come into contact with them. He used solutions of four degrees of strength; viz., one part of salicylic acid and one of spirit, in 120, 90, 60, and 40 parts of water. The first two (the weaker solutions) arrested the movements of the bacteria gradually; while, with the two stronger solutions, the arrest of movement was instantaneous. He has used salicylic acid locally and internally in two cases of diphtheria, he thinks with good effect; but he observes that more extensive observations are necessary in order to determine its value in this disease.—*Brit. Med. Journ.*, March 6, 1875.

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Bromide of Potassium in Migraine.—One of the essential characters of idiopathic migraine is its tendency to periodicity This periodicity may be variable, and is often monthly. The migraine is always at first hemicranial, and is then also accompanied by a peculiar sense of heaviness of one of the ocular globes or

over one of the orbits. It is this prodromic sensation that indicates the opportune time for efficacious intervention. Administered then, and in sufficient doses, the bromide of potassium arrests and prevents migraine, while, if the paroxysm is allowed to set in, it will scarcely mitigate the severity of the pain. It is from want of attention to this point that the bromide has caused disappointment. It should be administered in doses of from five to six grammes for an adult male, four for a woman, and from two to three for a child, given in a cup of well-sweetened infusion of lime-tree flowers, aromatized by a drop or two of mint oil. It should be swallowed by successive mouthfuls within a quarter of an hour or twenty minutes.—*Med. Times and Gaz.*, March 27, from *Rev. Méd.* March 1, 1875.

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Ergot in Acute Mania and other Hyperæmic States of the Nervous Centres.—Dr. Van Andel has employed a solution of ergotin in alcohol and glycerine hypodermically, in the above-named conditions, he states, with satisfactory results. The ergot he believes contracts the cerebral vessels.—*Schmidt's Jahrbücher*, Feb. 1875.

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Removal of Goitre; previous Ligature of Arteries.—On the 6th March, Mr. Lister removed a goitre from a young woman, by the method first adopted, we believe, by Dr. Heron Watson; namely, the four principal arteries of supply to the thyroid are tied through a central incision before the operation of removal is attempted. This has been found most effectual in restraining the hemorrhage, which, without this precaution, is so serious a drawback to the operation. The patient is doing very well.—*Brit. Med. Journ.*, March 13, 1875.

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Dangers of Forced Taxis.—M. Mollière, Surgeon of the Hotel-Dieu, Lyons, relates some cases in proof of the danger of this practice advocated by M. Gosselin and other Parisian surgeons. The taxis, he observes, is a most valuable procedure, which almost always succeds when employed immediately after strangulation, but only on the condition that it is not forced. If the hernia is susceptible of reduction, force is unnecessary, while this may prove most mischievous when the hernia cannot be reduced. By forced taxis we may succeed in reducing a hernia *en masse*, that should not be so reduced in consequence of the existence of intestinal perforations. Another danger (in exemplification of which M. Mollière relates some cases) arises from the ecchymoses and effusions of blood which ensue upon the manipulation, so that the operation has to be performed on parts that are far from their normal condition, and which, in consequence of the contusion of the tissues that has taken place, may easily pass into a state of gangrene, or be followed by symptoms of putrid infection. —*Med. Times and Gaz.*, April 10, from *Lyon Méd.*, April 4.

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A New Point in the Diagnosis of Ovarian Disease.—Mr. Spencer Wells mentioned, at a meeting of the Pathological Society (March 16), a very striking fact in illustration of the practical use of the microscope in the treatment and prognosis of disease. He has long taught that single cysts near the ovary may be truly ovarian (excessive growth of one Graafian follicle) or extra-ovarian (dilatation and growth of part of the remnants of Wolffian body, or parovarium) And he has found that, while the extra-ovarian cysts are often radically cured by a single tapping, the cyst contracting and never refilling, the true ovarian single cysts are almost certain to fill again. He had also shown that the contents of the parovarian cysts consisted of little more than pure water, with scarcely any albumen, or only a little albuminate of soda, the specific gravity seldom exceeding 1005. Mr. Thornton has recently discovered that the fluid in some single ovarian cysts contains little groups of cells, which he believes are only formed from the lining membrane of the Graafian follicle; and the presence of these cells, with the higher specific gravity and the amount of albumen or paralbumen in the fluid, are sufficient to enable a surgeon to say after tapping a single cyst whether it is likely to be radically cured by tapping only, or whether it is almost

certain to refill and require ovariotomy.—*Med. Times and Gaz.*, March 20, 1875.

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A New Form of Glycosuria.—M. Jules David has recently observed, in a case of diabetes following a large carbuncle, a substance resembling glucose in its reactions, but differing from anything hitherto discovered. The urine reduced Fehling's solution, although less readily than ordinary diabetic urine. Liquor potassæ also caused a brown discoloration. But on examining it with the polarizing saccharimeter, not the slightest trace of deviation could be discovered. Moreover, on analysis, no glucose whatever was found by the usual method. A solution of the substance, after isolation, reduced Fehling's solution readily. Its exact nature has not yet been determined, owing to failure of the supply of the urine.—*Lancet*, April 8, 1875.

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The Drawbacks of Esmarch's Ischæmia.—M. Dutrait, resident surgeon to the Charité Hospital of Lyons, has published an article on these drawbacks, which puts them in a very clear light (*Lyon Medical*, March 7th and 14th, 1875). Not that the author wishes to depreciate the method, which in many instances is extremely valuable; but his aim is to draw attention to the immediate loss of blood when the elastic compression is removed and all the vessels which could be found are tied, and to the secondary hemorrhage which not unfrequently occurs after ischæmic operations. M. Dutrait adduces many cases which have come under his own observation. They show that we should be very watchful against secondary hemorrhage. The author, to strengthen his position, alludes to a work lately published in Paris (Augier, De l'Anémie Artificielle), and to an article of M. Nicaise (*Gaz. Méd. de Paris*, Nov. 28, 1874), in which important statistics and excellent arguments will be found.—*Lancet*, April 8, 1875.

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Death from Chloroform.—A healthy man æt. 45, in the Adelaide Hospital, Australia, whose little finger required amputation, was given chloroform on Jan. 25th on a folded towel by one of the house-surgeons, who was thoroughly accustomed to the administration of the anæsthetic. The man struggled violently, and required to be forcibly restrained, when the face became livid, and he died, all efforts at resuscitation proving of no avail. This case resembles many other cases in the fact that the chloroform was administered for a trifling operation, and that death occurred in the struggling stage, when it may be supposed that the muscles were rigid, and that the danger of apnœa was superadded to that of chloroform narcosis. The anæsthetic was administered with all necessary caution and skill, and the jury returned a verdict in accordance with the facts, attaching no blame to anybody.—*Lancet*, April 8, 1875.

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Mode of Death from inhaling Nitrous Oxide.—M. Magitot asserts that the mechanism of death is very different from the employment of nitrous oxide, and from other anæsthetics, as ether and chloroform. When it results from the administration of the two latter, it is by syncope, but when from nitrous oxide, it is by pure and simple asphyxia.—*L'Union Médicale*, 16 March, 1875.

Mr. Darin, at a meeting of the Société de Chirurgie on March 3d, read a paper in which he denied that nitrous oxide acted by producing asphyxia. This gave rise to discussion, and the following conclusions were adopted: 1. That nitrous oxide does act by inducing asphyxia. 2. That ether has been too much neglected in Paris, and that it would be desirable to return to its employment.

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Mistaken Identity.—The following remarkable case is worthy of note, as an excellent illustration of the great caution which should be exercised in accepting evidence of identification: A short time ago an inquest was held on the body of an unknown woman, twenty-four years of age, which was taken from the river Avon, and bore such injuries as to lead the medical man to conclude, in the absence of all symptoms of drowning, that the deceased had been murdered and then placed in the river; and, after some evidence had been

received, the inquest was adjourned to allow of time for the collection of evidence as to who the unfortunate woman was, etc. On opening the adjourned inquest, the coroner commented upon the strange case of mistaken identity which had occurred in connection with the case. "In the course of their inquiries the Bath police met with a woman who stated that the description of the deceased corresponded with the appearance of a woman who had cohabited with a bargeman, and between whom there had been a violent disagreement at Bath, previous to their departure in the barge for Bristol. The woman was taken by the police to see the body, and, after a minute examination, which extended to a scrutiny of the teeth, the condition of which confirmed the woman in her opinion; she positively stated the deceased was the woman she referred to. This identification was positive, and was singularly correct both in general and in detail; and the woman's testimony would have been unhesitatingly placed before the jury for their acceptance but for one circumstance—the simple fact that the woman so carefully examined and so minutely described had been found alive." The lesson to juries and others is such a good one that it is worthy of being placed on record.—*Med. Times and Gaz.*, March 20, 1875.

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Dislocation of Humerus reduced after five months.—Dr. Thos. Gregg communicated, to the south of Ireland branch of the Brit. Assoc. (Feb. 10th), the case of a woman, admitted into the County Hospital on January 28th, the injury having occurred on September 5th previously, from an accident which was caused by a threshing-machine, in which her arm was caught. The nature of the injury was overlooked. On presenting herself at the hospital, the usual appearance of dislocation under the clavicle, the head of the bone being fixed in this position, and adherent to the neck of the scapula. On February 3d, the reduction was effected under chloroform, with pulleys. The usual apparatus employed in the hospital for old-standing luxations of the shoulder-joint, is a gutta-percha shield, which fits from spine to sternum, with an aperture for the arm, carefully moulded and bandaged to the body. This protecting shield affords a means of steadying the scapula, and also a fixing point for the straps, and preventing injury to the ribs. The President cited several instances of old standing luxation, which, he said, could not have been reduced but for the use of this gutta-percha-shield.—*Brit. Med. Journ.*, March 13, 1875.

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Hypospadias and Epispadias.—Mr. John Wood exhibited to the Royal Medical and Chirurgical Society (*Brit. Med. Journ.*, Feb. 13, 1875) some cases of hypospadias and epispadias, on which he had operated. Two of the cases of hypospadias were boys, who had been unable to pass urine in the erect position, but were obliged to crouch down. The stream passed backwards. Having previously, in other cases, tried various plans with unsatisfactory results, he adopted in these cases a plan which he had found successful in two other patients some time ago. It consisted in making a hole in the prepuce, through which the glans was made to protrude—the operation being somewhat the reverse of that of Nélaton for epispadias. At first, he did not realize the difficulty of properly adjusting the parts; but he succeeded by making a broad flap and turning the skin inwards to the urethra. The prepuce was utilized for the formation of a channel for the urine. In one of the cases, there was a *cul-de-sac* apparently representing the fossa navicularis, with the urethral opening behind it. This he slit up so as to form a continuous channel. In the case of epispadias, there was want of retention of urine; but this defect had been quite overcome by first operating by Néléton's method, and then performing two subsequent operations for completing the channel.

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International Medical Congress.—The fourth session of this Congress will commence at Brussels on the 19th of September next at noon, under the auspices of the government. Mr. Wleminckx, President of the Committee of Arrangements; M. Warlomont, Secretary.

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